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ABSTRACT

Coleman, Hoffer and Kilgore's claims regarding the effects of Catholic schools on cognitive achievement have evoked much controversy. Critics have argued that the cross-sectional testing data Coleman et al. used could not distinguish differential sector effectiveness from selection effects, i.e., that Catholic schools enroll students of superior academic competency. The First Follow-Up (1982) of the High School and Beyond Base Year Sophomore Cohort allows a stronger design for studying this issue. Using sophomore test performance to control for input-level differences in competency while predicting senior year test performance in several cognitive domains, the "common school" effect found by Coleman and his colleagues disappears. The omission of such input controls leads to a substantial upward bias in the estimate of Catholic sector effects on achievement. The best estimate of the Catholic sector effect on cognitive growth from the sophomore to senior year, using aggregate sophomore-to-senior year change in performance as a yardstick, is about two-thirds of a year's growth. Differences of this magnitude are judged to be substantively trivial because they correspond to less than .1 standard deviations in test performance. Sector differences in test performance are too small to warrant the attention they have received. (Author/BS)

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February 1984

SCHOOL SECTOR AND COGNITIVE PERFORMANCE: WHEN IS A LITTLE A LITTLE?

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Introductory Statement

The Center for Social Organization of Schools (CSOS) has two primary objectives: to develop a scientific knowledge of how schools affect their students, and to use this knowledge to develop better school practices and organization.

The Center works through three research programs to achieve its objectives:

The School Organization Program investigates how school and classroom organization affects student learning and other immediate outcomes of schooling. Current studies focus on parental involvement, microcomputers in schools, use of time in schools, cooperative learning, and other organizational strategies that alter the task, reward, authority and peer group structures in schools and classrooms.

The Education and Work Program examines the relationship between schooling and students' later-life occupational and educational successes. Current projects include studies of the competencies required in the workplace, the sources of training and experience that lead to employment, college students' major field choices, and employment of urban minority youth.

The Schools and Delinquency Program studies the problems of crime, violence, vandalism, and disorder in schools and the role that schools play in delinquency. Ongoing projects address the development of a theory of delinquent behavior, school effects on delinquency, and the evaluation of delinquency prevention programs in and out of schools.

CSOS also supports a Fellowships in Education Research program that provides opportunities for talented researchers to conduct and publish significant research in conjunction with the three research programs.

This report, prepared by the School Organization Program, uses longitudinal data to further examine the controversy about public-private school differences in effects on student test performance.

ABSTRACT

Coleman, Hoffer and Kilgore's claims regarding the effects of Catholic schools on cognitive achievement have evoked much controversy. Critics have argued that the cross-sectional testing data Coleman et. al. used could not distinguish differential sector effectiveness from selection effects -- i.e., that Catholic schools enroll students of superior academic competency. The First Follow-Up (1982) of the High School and Beyond Base Year Sophomore Cohort allows a stronger design for studying this issue. We use sophomore test performance to control for input-level differences in competency while predicting senior year test performance in several cognitive domains. The omission of such input controls leads to a substantial upward bias in the estimate of Catholic sector effects on achievement. We also show that the so-called "common school" effect found by Coleman and his colleagues disappears when appropriate input-level test controls are applied.

Our best estimate of the Catholic sector effect on cognitive growth from the sophomore to senior year, using aggregate sophomore-to-senior year change in performance as a yardstick, is about two-thirds of a year's growth. We judge differences of this magnitude to be substantively trivial because they correspond to less thar .1 standard deviations in test performance. We conclude that sector differences in test performance are too small to warrant the attention they have received.

Are private sector high schools more effective than public sector schools? As is well known, Coleman, Hoffer and Kilgore claimed they were in their 1981 analysis of the issue (1981a), and in so doing set off a vigorous and heated debate that has yet to be put to rest. Despite numerous analyses, re-analyses, critical exchanges and strongly held convictions, we are little closer to reaching agreement on the matter than when the issue first was joined. How can it be that so much concerted effort has shed so little light?

The problem, we believe, is that no one--neither Coleman and his colleagues, nor their many critics--has had access to the kind of data needed for a proper consideration of the question. Coleman, Hoffer and Kilgore compared the effectiveness of public and private schools with cross-sectional data from the High School and Beyond (HSB) project. The data set included scores for high school sophomores and seniors on a battery of tests developed for the project by the Educational Testing Service (Heyns and Hilton, 1982). Coleman, Hoffer and Kilgore judged differences in educational effectiveness by comparing the test scores of public school students with those obtained by private students after adjusting for social background characteristics. Since measures of test performance before high school were not available in the HSB data set, the researchers were not able to adjust as well for differences in student competency levels that might have predated high school.

Because of this design limitation, the fact that private sector students tend to score somewhat better than their public school counterparts on the HSB tests is equivocal as evidence of differential effectiveness. Does this pattern occur because private schools are better at promoting cognitive development, as Coleman, Hoffer and Kilgore conclude, or simply because such schools enroll better students in the first place? Since neither their data, their design, nor their analysis precludes the

latter possibility, many have deemed Coleman, Hoffer and Kilgore's evidence suspect and their conclusions unwarranted. From the critics' point of view, the effectiveness hypothesis has yet to be put to a proper test.

And so the debate is joined. In their defense, Coleman, Hoffer, and Kilgore point to their many efforts to work around the selection problem: their use of extensive socioeconomic controls as proxies for test performance; their attempt to study patterns of cognitive growth through something akin to a synthetic cohort analysis; and their experimentation with econometric methods for taking account of selection biases. The critics, in turn, have questioned the adequacy of these efforts. They also have proposed additional test score proxies and/or bases of comparison (e.g., controlling for track membership) that they think would improve upon those used by Coleman, Hoffer and Kilgore. Indeed, there now are available several analyses which employ such controls, and their use does tend to attenuate sector differences (Noell, 1982; Willms, 1982; Alexander and Pallas, 1983; Morgan, 1983).

Coleman, Hoffer and Kilgore, in turn, have argued that high school track is not a proper proxy for student competency differences. They likewise have rejected other published attempts to deal analytically with the selection issue when using cross-sectional data, such as Page and Keith's (1981) attempt to distinguish "ability" components in the HSB test battery from "achievement" components. With but few concessions to their protagonists, Coleman, Hoffer and Kilgore have held to their original claims.

This is where the debate stands at present. Neither side has produced the proverbial "smoking gun", and neither seems much impressed with the arguments advanced by the opposition. The reason for this stalemate, we believe, is that all the contending parties have had to argue around, and work within, the constraints of

cross-sectional data. Fortunately, new data now are available that should move the debate off stage center.

The HSB design made provision for the retesting of its original (i.e., 1980) sophomore sample two years later. This phase of the HSB fieldwork now is complete and the data recently have entered into the public domain. They hold great promise for clarifying the question that was first posed, but never satisfactorily addressed, by Coleman, Hoffer and Kilgore. We now can examine how individual patterns of test performance change between the sophomore and senior years, and whether such change is more pronounced among youngsters attending private schools than among those attending public ones. These data, then, allow for a direct test of the hypothesis that private schools are more effective than public in promoting cognitive development, with appropriate controls for input level test performance.

The present analysis parallels that used in our earlier examination of the cross-sectional HSB (and NLS Class of '72) data (Alexander and Pallas, 1983), except that sophomore test scores are used to adjust for "input level" competency differences and senior test scores are used as outcomes. Although we continue to think our earlier effort a worthwhile counterpoint to Coleman, Hoffer and Kilgore's excessive claims, we acknowledged then that it labored under the same design restrictions that hampered their original analysis. Our intentions in that piece were two-fold: to raise probing questions and to evaluate the tenability of their cross-sectional conclusions. The HSB panel data, though, should provide more secure answers to the original question of differential sector effectiveness.

The conceptualization which guides our analysis is depicted in Figure 1. It is very similar to that proposed by Kilgore (1983) in response to our earlier criticism of her work with Coleman and Hoffer. We indicated then that we had no particular

problems with this framework, but felt strongly that neither she nor we were able to do justice to it. With the HSB follow-up data, though, its implications can be evaluated properly.

-- Figure 1 About Here --

As in our earlier analysis, sector effects are estimated net of differences associated with various student characteristics (i.e., SES background, race and gender) and measures which tap regional differences. Here, though, we add sophomore test performance as another "input" control. One difference between this specification and our previous one is that now high school track membership (contrasting academic enrollment with general) intervenes between sector and the several domains of senior year test performance. Before we used high school track as a proxy for unmeasured cognitive differences, but this no longer is necessary since direct controls are available. Rather, we now entertain the possibility that track membership is important as an aspect of school organization. Coleman and his colleagues argued this in response to our analysis, as well as to others which used the track variable in this way (e.g., Morgan, 1983). They contended that channeling a large percentage of students into the college track was one of the ways private sector schools realized their superior performance.

The proper interpretation of the curriculum variable in these analyses is a matter of considerable importance, for sector differences within tracks generally are much smaller than those observed overall (Peng and Fetters, 1981; Willms, 1982; Alexander and Pallas, 1983). If one thinks of the track variable as a proxy for unmeasured student differences, then the available evidence offers little indication of differential sector effectiveness. If, on the other hand, tracking is important as a mechanism of school policy, then the differential effectiveness hypothesis receives

stronger support (although still in the absence of proper controls for student competency differences). Having available a baseline assessment of test performance, we now can grant Coleman, Hoffer and Kilgore their preference on the track membership issue. This framework, then, will isolate sector influences upon patterns of cognitive growth and the mediation of sector effects through patterns of curriculum placement.

As in our earlier study, we again limit our comparison to public and Catholic schools. This is because the HSB project included too few non-Catholic private schools to allow secure generalizations, a point conceded by Coleman, Hoffer and Kilgore (1981b; 1982b). We also have excluded vocational track students, owing to their sparse numbers in Catholic schools. Although this is not reflected in the diagram, all analyses are performed separately for public and Catholic schools so that interactions involving the sector distinction will not be obscured. Coleman, Hoffer and Kilgore have placed great importance on allowing for such interactions and have been especially critical of studies that fail to make provision for them (Coleman et al., 1982b; Coleman and Hoffer, 1983). Since one of our purposes is to evaluate the resilience of Coleman, Hoffer and Kilgore's conclusions, we thought it best to defer to their preference here as well, although we ourselves don't believe the matter to be as important in practice as they take it to be.

There is, though, another reason for proceeding in this way. Doing so allows us to reevaluate Coleman, Hoffer and Kilgore's claim that private sector schools better approximate the "common school" ideal than do those in the public sector. The basis for this conclusion is that differences in test performance associated with racial and ethnic group membership and with socioeconomic origins are less pronounced in the private sector. We, along with others (Bryk, 1981; Goldberger and Cain, 1982; Cain

and Goldberger, 1983), have expressed the concern that this pattern too might simply reflect selection differences (i.e., that private schools have a disproportionate enrollment of highly capable minority and disadvantaged youngsters). Hence, we will be interested to see how the inclusion of a suitable control for test score differences affects this detail of the results, as well as the more general matter of overall sector effects.

In the next section we review sample selection considerations, measurement procedures, and techniques of analy . Following this, we turn to the results themselves.

METHODS

This analysis uses data from the High School and Beyond 1980 Sophomore Cohort Base Year (1980) and First Follow-Up (1982) surveys. Our strategy shall be to use 1980 Sophomore test scores as input controls in equations predicting 1982 senior year test performance. A particularly important consideration in attempting to model sophomore to senior growth is how dropouts are handled. If, for instance, the dropout rate is higher in public schools, and dropping out is associated with poor test performance, then the public-Catholic sector difference in test performance likely would be attenuated. Coleman, Hoffer and Kilgore (1982a; 1982c) attempted to estimate sophomore-to-senior year learning rates using a synthetic cohort-like approach with adjustments for both student background differences and dropout differences, but those efforts have been received quite critically (Goldberger and Cain, 1982).

Fortunately, this problem is averted in the HSB panel, because dropouts were included in the first follow-up survey along with several other classes of students

(e.g., transfers). The Base Year sample of High School and Beyond consisted of roughly 30,000 sophomores in more than 1,000 high schools. Students still enrolled in their Base Year schools at the time of the First Follow-Up were sampled with a probability of 1.0. Students who were no longer attending their Base Year schools at the time of the First Follow-Up (i.e., dropouts, early graduates and transfers) were sampled at lower rates. The sample allocation consisted of 25,150 still-in-school seniors, 2,601 dropouts, 1,290 transfers to non-HSB schools, and 696 early high school graduates. Properly weighted, this sample projects to the population of roughly 3,800,000 high school sophomores of 1980. The HSB weighting factor for cases having both Base Year and Follow-Up testing data is used throughout our analyses. Further details on the sample and data collection may be found in Jones et al. (1983).

Questionnaire and testing data were collected as part of the first follow-up. The response rate for completed tests ranged from 78% for the dropout sample to 90% for the still-in-school sample. These figures compare favorably to the response rates of 77% for the Base Year sophomores and 72% for the Base Year seniors. In light of these figures, it appears that the HSB re-testing program was highly successful, and this simplifies considerably our analysis plan.

High School and Beyond administered the same battery of tests to the 1980 Sophomore Cohort in both the 1980 Base Year and the 1982 First Follow-Up phases of data collection. The areas covered in the tests are Vocabulary, Reading, Mathematics, Science, Writing and Civics Education. Mathematics is constructed as the sum of two mathematics subjects. Additionally, a composite score was constructed as the sum of the first three (i.e., Vocabulary, Reading and Mathematics). The Science, Writing and Civics tests are designed to measure curriculum-specific achievement. For all tests, we employ the formula scoring, which includes a correction for guessing.

Reliabilities for the sophomore tests, as reported by Heyns and Hilton (1982), range from .53 for Civics, to .85 for the Mathematics I test. From these figures, the estimated reliability for the Composite is .92. We assume the reliabilities to be constant across the two administrations, and hence apply the sophomore estimates to the senior tests as well. Further information on the HSB tests can be found in Heyns and Hilton (1982).

-- Table 1 About Here --

The other variables used in the analysis are taken either from the sophomore questionnaire or, in the case of our school location measures, from the stratifying information used in drawing the HSB sample of schools. As student background controls, we include measures of family SES, of the students' race/ethnicity and of the students' sex. The sources and coding of these data are summarized in Table 1.

RESULTS

To determine whether Catholic schools produce superior test performance, we performed several within-sector regression analyses. These differed from one another in the mix of control variables they included. The weighted N varies across dependent variables and ranges from 2,338,917 to 2,501,801 in the public sector equations, and from 209,487 to 211,814 for the Catholic sector equations.

By performing separate analyses for public and Catholic schools, the effects of our control variables are allowed to differ in the two contexts. Coleman, Hoffer and Kilgore have concluded that such interactions are quite important (they are the basis of their "common school" interpretation), and they have been especially critical of analyses which failed to make provision for them (e.g., Noell, 1982).

A "standard" set of controls is included in all of our estimations. This includes selected student background characteristics (i.e., race, gender, and family socioeconomic level) and variables that capture locational differences in the distribution of schools (i.e., region of the country). These are the same "input" controls used in our earlier analysis of the senior year cross-sectional data (Alexander and Pallas, 1983), and they are reasonably reflective of the control strategies used in this literature generally. Adopting this same approach as the baseline for the present inquiry will allow us to judge its adequacy as an analysis strategy. This is potentially quite important, for virtually all studies to date have had to rely on this general sort of approach.

For our major substantive analysis, which we report first, sophomore test scores are added to this set of background controls. Since the HSB project administered parallel test batteries in the sophomore and senior years, we are in the fortunate position of being able to adjust for prior levels of performance in the same cognitive domain before judging sector differences. Despite our many other differences, the contending parties to this debate at least have agreed that this is the preferred way to study school effects on cognitive outcomes (Alexander and Pallas, 1982; Coleman, 1982).

Although this probably is the strongest non-experimental design for investigating patterns of cognitive development between the sophomore and senior years of high school, one potential problem is that is neglects sector differences which express themselves in patterns of performance through the Spring of the sophomore year. More will be said of this later, but for now we wish simply to emphasize that our analysis looks forward from the sophomore year. Of course, it would be a peculiar sort of effective school which exhausted its impact in the first year or two,

especially since these years presumably are devoted primarily to lower level materials. Because of such considerations, we aren't especially troubled by this design limitation. Neither, we gather, are Coleman and his colleagues, since they went to great lengths to approximate such a change analysis when neither design nor data were especially well suited to the task.

In the analyses which follow, we have corrected all regression results for the attenuation induced by random errors of measurement in the cognitive performance measures. Corrections are employed for both the sophomore year input test controls and the senior year criterion measures. Our analyses implicitly assume that all other measures in the analyses are perfectly reliable. Computationally, the attenuation correction involves dividing the elements of each within-sector zero-order correlation matrix by the square root of the product of the estimated reliabilities of the variables referenced by the element (Nunnally, 1977).

Table 2 presents our best estimates of how school sector conditions cognitive development between the sophomore and senior years. Because these sector differences are derived with the sophomore year counterparts of the outcome measures controlled, the results indicate whether cognitive growth (in the sense of improved levels of performance) actually is more pronounced among private sector students. Being able to take account of "input-level" performance differences in this way should allay most concerns regarding selection biases.

However, there still is one way in which selection differences could be confounded with the sector distinction, and this at least deserves mention. If youngsters in the two sectors were developing cognitively at different rates at the time of our initial observations (i.e., toward the end of the sophomore year) and this difference persisted throughout the time frame under consideration, then these

different developmental patterns would show up in our analysis as differential sector growth even though they might have little (or nothing) to do with organizational influences. In fact, because of this potential problem, Coleman, Hoffer and Kilgore, in an addendum to their book (1982c: 209), draw the following conclusion regarding the promise of such panel assessments: "Thus, 'selection bias' haunts longitudinal studies almost as much as it does cross-sectional studies."

This statement probably is true as far as it goes, but it doesn't go very far. If there is reason to suspect differential learning rates across sectors, then it likely will be the higher performing students who progress at the faster pace (Werts and Hilton, 1977). Since Coleman, Hoffer and Kilgore already have concluded that it is private school youngsters who perform better, then such bias probably would favor the private sector. Olneck (1981), in fact, makes this point in his commentary on Coleman, Hoffer and Kilgore's research.

This, then, is yet another reason to be skeptical regarding Coleman, Hoffer and Kilgore's original results, and they are quite correct that it could complicate panel assessments as well. But the concern with their analysis is that it might overstate the Catholic school advantage. Being able to control explicitly for input level test performance surely must be judged an improvement over estimations which do not employ such controls. The fact that results from such an approach might still be biased in favor of the private sector is something to be mindful of in making sense of the results, not a reason to forego an improved evaluation.

We grant that longitudinal analysis is no panacea, but at least in this instance we can be reasonably confident as to the likely consequences of those complications we cannot bring under control. These suggest an important caution in interpreting any private sector advantage, but they give no reason to suspect our analysis will

understate that advantage. This would be our major concern, since in re evaluating Coleman, Hoffer and Kilgore's conclusions we want to be especially careful not to err in the other direction.

In Table 2, we include three alternative representations of the estimated sector differences. They are entirely equivalent, but differ in their frame of reference. To generate the figures in Table 2, the paired public and Catholic equations for each outcome were evaluated by means of a regression standardization (Althauser and Wigler, 1972; Iams and Thornton, 1976). Actually, two such standardizations were performed. The first calculated predicted outcome levels by applying public school predictor means to the separate sector parameter estimates. These are the values reported in Table 2. This approach derives the levels of test performance that would be expected if schools in the two sectors enrolled students similar to those presently attending public schools and if both sets of schools were distributed regionally like the present mix of public schools.

A second set of calculations also was done which parallels the first, but uses Catholic school means throughout. This approach, then, asks how large the differences would be if schools in the two sectors enrolled students like the ones that now attend Catholic schools (and if they were distributed regionally as Catholic schools). These results (along with those highlighted in Table 2) will be presented in Table 3, in which several issues of secondary importance are considered. For now, though, we focus on the differences that are observed at the public school means, because these correspond most closely to the sort of policy change contemplated by Coleman, Hoffer and Kilgore -- that is, broadening access to the private sector for the kinds of students who presently attend public schools. Although the pattern of results is not very different under the two assessments, sector differences consistently are smaller

when using the Catholic school means. Coleman, Hoffer and Kilgore observed a similar pattern.

The first column in Table 2 presents the results of these computations at the public school means as simple formula score differences. Although it is difficult to judge the significance of these figures without a frame of reference that provides a tangible anchor, we suppose it is of some interest that all the differences, except that for the composite, are well below a single item. More informative, though, are the representations of these differences in the second and third columns.

-- Table 2 About Here --

The second set of entries expresses these differences as fractions of a year's growth. This same standard was used by Coleman, Hoffer and Kilgore in much of their work, and is the basis for their summary generalization that Catholic schools perform about a year above public schools. In their analysis, the estimate of a "year's growth" was computed as half the background adjusted difference in average performance levels between the public school sophomore and senior samples. Our figures, on the other hand, reflect patterns of individual change over the two year interval covered by our panel. They are simply half the difference between 1980 and 1982 test means. The entries in the second column express the formula score sector difference as a fraction of this estimate of a year's average growth. For only one of the seven evaluations does the difference reach Coleman, Hoffer and Kilgore's standard of a year, this being on the reading test. For both of the other two tests which tap more generic competencies, the sector difference is well below this level, being only about two-thirds of a year for vocabulary and merely a third of a year for mathematics.

The last three tests are subject specific assessments. These cover science, writing and civics. Several critics (Heyns and Hilton, 1982; Goldberger and Cain,

1982) have argued that such curriculum-linked tests would be a more proper basis for judging school effectiveness, since they tap knowledge that presemably is acquired largely in school. Although these tests were available for the HSB sophomores in the base-year of the project, Coleman, Hoffer and Kilgore (1982b) declined to consider them because comparison tests were not available for the Base Year senior cohort. They were re-administered during the first follow-up, however, and we are able to include them in the present analysis.

The results for these three subject-specific tests differ from those for the first four tests. The difference on the writing test is largest of the three. At .64 years, it is about as large as the vocabulary difference. The other two, though, both are much smaller, with that for Civics actually favoring the public schools. These figures, then, offer no reason to think that Catholic schools do especially well on curriculum-linked tests. In fact, their advantage, if anything, is smaller on these assessments, a pattern that is contrary to expectation if organizational efficacy is what really is at issue.

While these differences generally are a good bit smaller than those reported by Coleman, Hoffer and Kilgore, it is difficult still to know whether they are large or small in any absolute sense. Coleman, Hoffer and Kilgore present their finding of about a year's difference as though it were a sizable gap, but they don't tell us the basis for this judgment and just a bit earlier in their book they discuss the two year sophomore to senior public school difference (which is the basis of the year benchmark) as though it were rather small. Is half a small difference large enough to be deemed important? They seem to believe so, but fail to say why.

Setting aside the Civics results, a reasonable summary for our data probably would be close to two-thirds of a year, which would correspond to about a third of a

small difference. Again, is this large enough to justify the conclusion of an important Catholic school advantage? The standard of a year's growth isn't very helpful here, and it is for this reason that we prefer the base of comparison built into the third mode of presentation.

The last column expresses the estimated sector differences as fractions of the test battery standard deviation. Although evaluating the sector difference against the variability in the test itself still does not afford an absolute standard, it at least provides an internally consistent frame of reference. From these comparisons we can judge the average test score difference across sectors against the variability in the unit.

This seems to us to be the best of the less than ideal options that are available, and by this standard the sector differences quite clearly are small. Only two differences are even as large as a tenth of a standard deviation, itself a very modest threshold. These are for the vocabulary and writing tests. For the two other domains studied by Coleman, Hoffer and Kilgore -- Reading and Math -- the differences both are but six-hundredths of a standard deviation. As before, the figures for the subject specific tests are even smaller.

Incidentally, when Coleman, Hoffer and Kilgore's corresponding cross-sectional results are considered in like fashion, the differences range from .144 to .189 standard deviations for the sophomore cohort and from .114 to 284 standard deviations in the senior analysis (see Table 5, below). Clearly, then, our panel analysis indicates a considerably smaller Catholic school advantage, either when judged as fractions of a year's growth or as fractions of the test standard deviation. This pattern of differences seems to substantiate the concern expressed by many that Coleman, Hoffer and Kilgore's design and analysis likely overstate the performance

difference between public sector and private sector youngsters that is attributable to organizational factors. Our own evaluation of the sector effects in Table 2 is that differences below a tenth of a standard deviation are too small to be considered of practical importance. Since judgments of substantive importance are, we believe, the central consideration, this pattern of results not only scales down Coleman, Hoffer and Kilgore's estimates of sector impact, but it also overturns their general conclusion.

As mentioned above, the results in Table 2 are our major substantive conclusions. However, several subsidiary issues that arose repeatedly in the exchanges between Coleman, Hoffer and Kilgore and their critics are considered in Table 3. In this table, comparisons are reported at both the public school means and the Catholic school means, and the figures from Table 2 are reported as the first row of results to serve as a base of comparison with the others. To simplify matters, we only report sector differences as fractions of the test standard deviation. This corresponds to the third column of results in Table 2.

-- Table 3 About Here --

The first issue considered in Table 3 is the adequacy of background measures as proxies for test score controls. It will be recalled that Coleman, Hoffer and Kilgore had to rely upon such surrogates. Despite the reservations voiced by many, they have held to the position that these served their purposes adequately. We are able to evaluate this by estimating sector differences using only background controls (the second row of results), and then comparing these against those obtained when test controls are added to the analysis (our substantive results, reported in the first row).

If Coleman, Hoffer and Kilgore are correct in their opinion that background controls adjust adequately for selection differences, then the two sets of results should line up rather closely. Clearly though, they do not. In fact, the differences generally are quite substantial, with the background-adjusted estimates usually being from two to four times the value obtained with test controls. Bias of this magnitude could hardly be considered negligible. These comparisons demonstrate again (e.g., Alexander, McPartland and Cook, 1981) that background proxies simply are inadequate when attempting to assess school organization impact on cognitive outcomes.

The second issue considered in Table 3 is whether curriculum placement plays an important role in mediating the (small) private sector advantage obtained in our substantive analysis. It will be recalled that Coleman, Hoffer and Kilgore argued, in response to those who proposed using the curriculum variable as a proxy for unmeasured student differences, that sector differences in curriculum placement are a result of school policy differences, and hence that curriculum is important as an organizational variable. We now are in a position to evaluate how much of the sector effect actually is mediated through curriculum placement.

This is accomplished by adding curriculum to the analysis and comparing the sector differences that are obtained when curriculum is not controlled (row 1) with those estimated with curriculum controlled (row 3). The difference between these two estimations would reflect the anticipated mediation. Most of the estimates, though, are unaffected by the addition of curriculum, and where changes do occur they are extremely slight. Hence, we see little indication that private sector schooling works its magic by channeling a disproportionate number of its students into the academic curriculum. Of course, the adjusted sector differences themselves were so slight that there wasn't much for the curriculum variable to mediate in the first place. Hence,

we find little support for Coleman, Hoffer and Kilgore's argument that sector differences in tracking policy are an important organizational means by which private schools promote high levels of test performance.

The last issue considered in Table 3 is how well the curriculum variable served the interests of Coleman, Hoffer and Kilgore's critics who argued that its use in the absence of test score controls would reduce selection biases. This can be judged by comparing the fourth row of results, which adds curriculum to the analysis but drops the test control, with the first, which is the preferred approach (i.e., uses test controls, and not curriculum), and with the second (which uses only background controls). Here again, the pattern seems rather clear. Although the estimated sector differences when curriculum is used as a proxy for testing data are generally a good bit larger, they at least are a notable improvement over the figures derived when background controls alone are used. It thus seems clear that the curriculum-controlled figures are preferable to the background adjusted figures, but we don't take much solace in having been so vindicated. What we hadn't realized when urging the merits of the curriculum proxy was how far off the mark its use still left us. Fortunately, with suitable test score controls now available, we no longer need rely on this patchwork framework.

The last issue we consider is whether achievement processes in Catholic schools better approximate the common school ideal than do those in public schools. Coleman and his colleagues contend they do based on their finding that test scores are less strongly related to student background characteristics in the private sector. But since these results too were derived without proper controls for prior test performance, they also should be deemed provisional.

The detailed regression results behind the comparisons in rows one and two of Table 3 speak to this issue. It will be recalled that the first of these estimations had test controls, while the second did not. Hence, differences in the background coefficients between the second and the first will reveal whatever distortion might be due to the omitted test control.

-- Table 4 About Here --

Table 4 presents these background coefficients. The first two columns are from the public and Catholic equations when sophomore test performance is not controlled. The third column is the simple difference between them. The last three columns are the corresponding figures for the equations which include test scores. To highlight the issue, comparisons in which the Catholic coefficients are larger than their public sector counterparts have been set to zero.

It is apparent from these figures that support for the common school hypothesis evaporates under the second specification. In eighteen of the twenty-one comparisons from the first analysis the differences are in line with Coleman, Hoffer and Kilgore's expectations. That is, the coefficients are smaller in the Catholic equations. When sophomore test scores are controlled, the pattern changes dramatically. In the second instance, seventeen of the twenty-one differences actually favor the public schools, and the remaining differences have shrunk considerably from their original values.

We conclude, then, that the relationship of student background characteristics to patterns of cognitive development between the sophomore and senior years is very similar in public and Catholic schools. It is not the case, then, that minority youngsters and students from low SES households are more successful in private schools when compared against equivalently competent public school students. Here too, Coleman, Hoffer and Kilgore are not sustained.

DISCUSSION

To recapitulate, our reading of the results in Tables 2, 3 and 4 suggests two main conclusions. First, and most importantly, there is little support for the notion that cognitive development in Catholic schools outpaces that in public schools between the sophomore and senior years. Our second conclusion is that background characteristics relate to test performance in similar fashion in the public and Catholic sectors. We therefore find no support for Coleman, Hoffer and Kilgore's "common school" hypothesis that minority and disadvantaged youngsters are especially well served by private sector schooling. Our remaining comments elaborate upon the first of these two conclusions.

Our analysis does indicate a small Catholic sector advantage for most outcomes. It is somewhat larger for more generic competencies than for subject-specific assessments, but even in the former areas most differences are less than a tenth of a standard deviation. These differences correspond to about two-thirds of a year's growth, on average, but some differences are a good bit smaller.

It turns out that these estimations are not all that different than those previously reported by Coleman, Hoffer and Kilgore and others, but under deficient designs. To be sure, our specific figures are lower, but not dramatically so. In fact, much of the debate over the matter has hinged, we believe, on different standards of judgment, which too often are only implicit.

Anyone who has tried to stay abreast of these ongoing exchanges knows how dense and detailed they can become. Much of the argument strikes us as misplaced, such as quibbling over the correct standard error to be used in determining statistical

significance. With an implied sample size in the millions, that judgments might hinge on alpha levels itself makes the case that there is not much going on. Kilgore's recent (1984) statement that she expects the Catholic sector advantage to be positive and significant when properly evaluated similarly begs the issue. Substantive importance, rather than statistical significance, should be our concern, and here the debate offers little constructive guidance.

From the outset, we have been of the opinion that the sector differences observed by Coleman, Hoffer and Kilgore were too small to warrant much attention. The fact that their figures likely were overly generous due to design weaknesses simply reinforced that belief. Indeed, we began our response (Alexander and Pallas, 1984) to Kilgore's critique (1983) of our earlier analysis as follows: "We agree with Kilgore on only one point: that the results we present are not all that dissimilar from those reported by her and her colleagues. What is striking is that we read their implications so differently." Her rebuttal (1984), in turn, is entitled "Schooling Effects: When is a Little Alot?" We apparently agree that these effects are little. We differ, though, on whether they are properly deemed "alot". A bit of history is relevant here.

Coleman, Hoffer and Kilgore have used various strategies to estimate sector effects on cognitive achievement. They have examined cross-sectional differences adjusted for student background (1982c), and "change" scores comparing the Base Year sophomore and senior cohorts, controlling for social background, as well as adjusted for dropouts (1982c). They have also examined learning rates, adjusted for background and also for dropouts (1982c). Additionally, they have assessed sector differences in specific "organizational" predictors of test performance (1982), and they have controlled or adjusted for curriculum (1981b; 1983; 1984). These diverse strategies

make comparisons across analyses quite difficult, as the units in which effects are expressed often change along with the approach.

We have tried to convert their main results into a common metric, either a fraction of a year's growth or a fraction of the criterion standard deviation. These comparisons are summarized in Table 5 (construction of the table is described in detail in Alexander, 1984).

-- Table 5 About Here --

Considered side-by-side and in comparable terms, there is a good deal of variability in Coleman, Hoffer and Kilgore's own evidence regarding the supposed Catholic advantage in vocabulary, reading and math. In their major statement (1982c: 141), they conclude that Catholic schools surpass public schools by about one grade level after adjusting for socioeconomic differences. However, this difference is observed consistently only for the sophomore cohort cross-sectional analysis. The results for their "change" analyses all fall far short of the standard of a year's growth, and those from their third analysis, which works through the implications of differences in specific school resources and experiences, fluctuate in size across performance domains and in pattern across cohorts. Moreover, in comparing across analyses we observe many curious inconsistencies. Hence, their original analyses are inconsistent in the advantage they attribute to Catholic schools, with the differences frequently falling below their standard of a year.

Also, their more recent statements on the magnitude of sector differences have modified downward this initial claim. In Kilgore's first response to our cross-sectional analysis, she reports what she believes are preferable curriculum-adjusted figures. These credit the private sector with some of curriculum's influence because private school policy serves to channel more youngsters into the academic

track. Although we are not much impressed with the basis for these calculations (Alexander and Pallas, 1984), they nevertheless lead Kilgore (1983: 185) to conclude that Catholic schools produce "at least a one grade level advantage in mathematics and vocabulary, and slightly less than one grade level in reading." This characterization seems to us a bit weaker than in the original volume in that it explicitly exempts reading from the one grade level standard (although some statements in the book include this qualification, it is not carried along consistently in their summary statements).

The latest recasting of their findings appears in Kilgore's (1984) second rebuttal to our SOE article. Acknowledging the need for a better yardstick by which to gauge the magnitude and meaning of sector differences, she turns to Jencks and Brown's (1975) estimate of learning rates between the ninth and twelfth grades based on Project Talent panel data. Assuming an annual learning rate of .22 standard deviations, Kilgore computes a Catholic sector advantage of .68 grade levels in mathematics, .77 grade levels in reading, and 1.41 grade levels in vocabulary. Rather than an advantage of "at least a year" in all domains, this claim now applies only to performance on the vocabulary test.

The above, we believe, summarizes what Coleman, Hoffer and Kilgore have had to say regarding public-Catholic differences. Our review is sketchy on technical detail, but it is accurate nevertheless. We also have withheld evaluative comment. Our intention is not to critique these various presentations, but simply to lay them out so that we can properly judge how well our results correspond to theirs. We find a Catholic sector advantage that averages about two-thirds of a year across several cognitive tests. This is quite close to Kilgore's most recent claim for two of the three tests that they considered in their work.

It seems, then, that after several rounds of reflection (and some re-analysis), Coleman, Hoffer, and Kilgore's more modest appraisal of their evidence lines up reasonably well at with at least some of the results from our analysis of change. Using the yardstick borrowed from Jencks and Brown, though, Kilgore goes on to conclude that these grade level equivalency differences "are not small." She thus seems to think that casting her results in terms of grade level equivalencies provides a basis for deeming "a little" to be "alot." We're not sure why this should be the case (her comment offers no explanation), but we judge our estimate of .67 grade level differences to be simply "little", and far too little to warrant Coleman, Hoffer and Kilgore's indictment of public sector schooling as inferior to that in the Catholic sector. We judge these small because they correspond to differences that are less than .1 test score standard deviations. Hence, the difference in test performance that can be attributed to sector differences is minute relative to the variability in cognitive skills. Sector effects simply don't matter much, and talking about them as though they did is both poor science and a poor basis for informing public policy. These were our earlier conclusions, and further analysis seems to sustain them.

Finally, in the introduction to the present analysis we acknowledged a lingering uncertainty. Our results do not address the possibility of sector effects on test scores through the tenth grade. Coleman, Hoffer and Kilgore studied sophomore patterns of test performance and interpreted them as reflecting differential sector effectiveness; their critics, ourselves included, felt they more likely were due to selection differences. Neither they nor their critics could muster conclusive evidence on the issue, and this remains the case still. Lacking a proper evaluation, we therefore must continue to rely on circumstantial indications and reasoned judgment.

Consider the figures reported in Table 6. These are zero-order correlations relating background characteristics to each other as well as to the test battery composite. Results for the other tests would be quite similar. We focus first on the zero-order associations between background and sophomore test performance across the two sectors. These are smaller in the Catholic sector than in the public sector. Coleman, Hoffer and Kilgore interpret the lower Catholic sector correlations between the background characteristics of SES and race/ethnicity, on the one hand, and test scores, on the other, as evidence for their common school hypothesis. Of course, they focused on how these correlations expressed themselves in a regression analysis, but this is incidental to our point. Others suspected these differences simply reflected that low SES and minority youngsters who attended private schools were somewhat exceptional in the first place. The correlations in Table 6 relating background and test performance in the two sectors do not distinguish between these two interpretations. They could have been generated either by differential selection processes or by differential sector effectiveness.

-- Table 6 About Here --

Other correlations in Table 6, though, are unambiguously the result of selection processes. In particular, the correlations between minority group status and SES level necessarily reflect selection differences. Table 6 shows a sizable negative association between race and SES in the public sector, consistent with our knowledge about the relative ranking of minorities in American society generally. In the Catholic sector, however, there is virtually no correlation between race and SES. This is a peculiar pattern to be sure.

While it at least is conceivable that private school organization could operate to attenuate the link between background and test performance, it is highly dubious

that any sort of organizational intervention could alter the background connection between minority status and socioeconomic disadvantage. It is striking, therefore, that SES and minority status should be so loosely coupled among Catholic school youth. What we see, then, are selection differences that are of the same order of magnitude as the test score correlation differences. Clearly, selection differences could be large enough to account for the sophomore "common school" pattern. This is hardly conclusive, but to us it at least is suggestive. The burden of proof continues to rest with those who favor the effectiveness hypothesis, and it seems to us a considerable burden indeed.

Rather than persist in the elusive quest for substantial private sector effects on cognitve performance, a more constructive agenda for the future might well be to ponder why it is that private schools don't outpace public institutions by the sort of margin that their many advantages would seem to anticipate. The pattern of results regarding public-Catholic differences do indeed strike us as provocative (and potentially important), but not at all for the reasons given by Coleman, Hoffer and Kilgore.

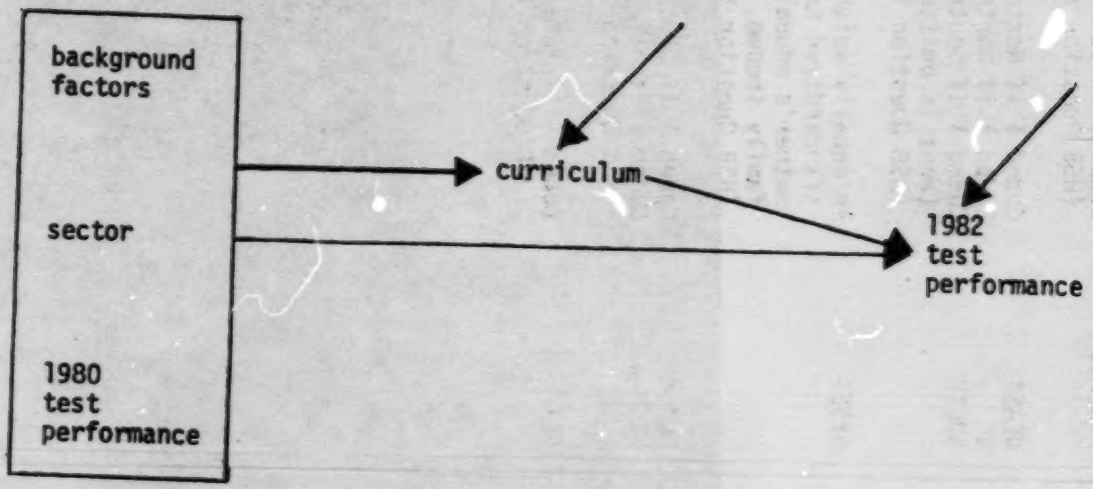

Figure 1. A Mcdel of Cognitive Achievement

TABLE 1

INDEPENDENT VARIABLES USED IN REGRESSION ANALYSES

Variable	Variable Name	Codes and Sources
School sector	PUBPRIV	Coded 1 if Catholic; 0 if public. (HSB Question FLAG17)
Region	NEAST	Coded 1 if Northeast; 0 otherwise.
	NC	Coded 1 if North Central; 0 otherwise.
	SOUTH	Coded 1 if South; 0 otherwise. (West is omitted category) (HSB Question FLAG19)
SES	BYSES	An equally weighted linear composite of standardized measures of father's education, mother's education, father's occupation, family income, and household items. (HSB Question FLAG25)
Race/ethnicity	BLACK	Coded 1 if black; 0 otherwise.
	HISPANIC	Coded 1 if Hispanic; 0 otherwise. (HSB Question FLAG22)
Sex	SEX	Coded 1 if female; 0 if male. (HSB Question FLAG21)

Table 2

Estimates of Sector Differences in Sophomore to Senior Patterns of Test Score Change from the HSB Panel[a]

	Estimated Formula Score Difference	As Fractions of a Year's Growth	As Fractions of the Test Standard Deviation
Composite	1.649	.66	.08
Reading	.665	1.13	.06
Vocabulary	.707	.69	.12
Mathematics	.311	.36	.06
Science	.125	.29	.03
Writing	.509	.64	.10
Civics	-.029	-.05	-.01

a. Figures are derived from parallel regressions which have been evaluated at the public school means. The predictors used are three dummy variables for region of the country, a dummy variable distinguishing blacks from whites, a dummy variable distinguishing Hispanic youngsters from non-Hispanics, a sex code, an SES composite, and the sophomore year counterpart of the outcome test. All correlations involving tests have been corrected for random error using reliability estimates reported in Heyns and Hilton (1982).

Table 3

Adjusted Sector Differences in Test Outcomes, with Tests Corrected for Attenuation

		Evaluated at Public Means	Evaluated at Catholic Means
COMPOSITE			
	(1)	.08	.06
	(2)	.30	.18
	(3)	.08	.06
	(4)	.22	.09
VOCABULARY			
	(1)	.12	.05
	(2)	.32	.19
	(3)	.12	.05
	(4)	.25	.12
READING			
	(1)	.06	.04
	(2)	.22	.12
	(3)	.04	.03
	(4)	.13	.05
MATH			
	(1)	.06	.07
	(2)	.27	.15
	(3)	.05	.06
	(4)	.19	.06
SCIENCE			
	(1)	.03	.04
	(2)	.09	-.01
	(3)	.03	.03
	(4)	.02	-.08
WRITING			
	(1)	.10	.04
	(2)	.27	.17
	(3)	.09	.03
	(4)	.21	.10
CIVICS			
	(1)	-.01	-.03
	(2)	.16	.04
	(3)	-.02	-.04
	(4)	.08	-.04

Note: (1)=Background controls and input test control; (2)=Background controls; (3)= Background controls and input test control and curriculum; (4)=Background controls and curriculum.

The adjustment involves a regression standardization in which the following background controls are used: NEAST, NC, SOUTH, BLACK, HISPANIC, SEX, BYSES. Regressions are corrected for unreliability in test score predictors and outcomes.

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Table 4
Within-Sector Background Effects on Test Performance
With and Without Input Test Controls

	(I) PUBLIC BACKGROUND CONTROLS ONLY	(II) CATHOLIC BACKGROUND CONTROLS ONLY	(I-II)	(III) PUBLIC BACKGROUND AND INPUT TEST	(IV) CATHOLIC BACKGROUND AND INPUT TEST	(III-IV)
TEST82						
BLACK	-10.287	-9.636	.651	.352	-.312	0
HISPANIC	-11.202	-9.624	1.578	-.733	-1.108	0
BYSES	10.141	5.995	4.146	.844	1.245	0
R^2	.282	.154		.916	.875	
VOCABULARY						
BLACK	-3.428	-2.680	.748	-.187	-.049	.138
HISPANIC	-3.296	-2.736	-.560	-.293	-.674	0
BYSES	2.934	1.675	1.259	.264	.109	.155
R^2	.290	.157		.937	.867	
READING						
BLACK	-2.540	-1.724	.816	-.238	-.227	.011
HISPANIC	-2.764	-2.219	.545	-.137	-.518	0
BYSES	2.365	1.468	.897	.212	.265	0
R^2	.212	.104		.871	.829	
MATH						
BLACK	-5.187	-5.776	0	.366	-.097	0
HISPANIC	-5.733	-5.233	.500	-.502	-.194	.306
BYSES	5.311	3.160	2.151	.536	1.107	0
R^2	.253	.141		.921	.862	
SCIENCE						
BLACK	-3.509	-3.371	.138	.004	-1.288	0
HISPANIC	-3.102	-2.185	.917	-.197	-.565	0
BYSES	1.958	1.400	.558	.067	.459	0
R^2	.297	.180		.933	.817	
WRITING						
BLACK	-2.823	-2.115	.708	-.226	-.279	0
HISPANIC	-2.795	-2.447	.348	-.268	-.678	0
BYSES	2.051	1.106	.945	.248	.339	0
R^2	.264	.178		.801	.717	
CIVICS						
BLACK	-.992	-1.035	0	-.273	-.595	0
HISPANIC	-1.389	-1.602	0	-.328	-1.006	0
BYSES	1.249	.699	.550	.136	.318	0
R^2	.206	.127		.861	.715	

a) Figures in which the Catholic sector coefficients are larger than their public sector counterparts have been set to zero.

Table 5

Public-Catholic Test Score Differences from Coleman, Hoffer and Kilgore's Three Analyses[a]

As Fractions of a Year's Growth[b]

	A. Cross-Sectional Results		B. Synthetic Cohort Results[e]		C. School-Based[f] Predictor Results	
	Sophomore[c]	Senior[d]	Background Adjusted	Dropout Adjusted	Sophomore	Senior
Reading	.876	.657	0.00	0.00	1.04	.767
Vocabulary	1.14	1.78	.603	.635	.286	.159
Mathematics	1.32	1.36	.023	.682	1.39	3.20

As Fractions of the Test Battery Standard Deviation

	A. Cross-Sectional Results		B. Synthetic Cohort Results[g]		C. School-Based Predictor Results	
	Sophomore	Senior	Background Adjusted	Dropout Adjusted	Sophomore	Senior
Reading	.159	.114	.000	.000	.189	.133
Vocabulary	.189	.284	.096	.102	.047	.025
Mathematics	.144	.141	.002	.071	.151	.333

a. The estimates in A and B are derived from parallel regressions which have been evaluated at the public school means.

b. The frame of reference ot determining a year's growth was one-half the adjusted public school sophomore t senior change, reported by Coleman, Hoffer and Kilgore in Table 6-7, P. 138. The figures used are: .365 for Reading; .315 for Vocabulary; and, .44 for Mathematics, as reported in Table 1, above.

c. The adjusted raw score differences from which these figures were derived are reported in Coleman, Hoffer and Kilgore's Table 6-7, P. 138. They were obtained by applying the Catholic means on the school-based predictors to public equation.

d. The adjusted raw score differences from which these figures were derived are reported in Coleman, Hoffer and Kilgore's Table 6-21, P. 171. Apparently these comparisons are adjusted to the family background characteristics of public school sophomores.

e. The adjusted raw score differences from which these figures were derived are reported in Coleman, Hoffer and Kilgore's Table 6-8, P. 142, and Table 6-11, P. 149.

f. The estimated changes in public school performance from which these figures were derived are reported in Coleman, Hoffer and Kilgore's Table 6-21, P. 171.

g. The pooled senior year standard deviations reported by Coleman, Hoffer and Kilgore in Table 6-3, p.127 were used to compute these entries.

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Table 6

Zero-Order Correlations of Sophomore Test Performance, SES and Race/Ethnicity, Separately by Sector*

	PUBLIC		CATHOLIC	
	1980 COMPOSITE	BYSES	1980 COMPOSITE	BYSES
BYSES	.453		.254	
BLACK	-.247	-.193	-.138	-.052
HISPANIC	-.231	-.165	-.190	-.142

*Note: Test correlations are corrected for attenuation.

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